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REFLECTIONS ON NAMING AND OTHER SYMBOLIC BEHAVIOR

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To deal with issues raised in the foregoing commentaries, it may help to sketch the origins and nature of our enterprise, because our theoretical concerns go back some time and extend over a range of behavioral phenomena quite apart from stimulus equivalence. Much of our earlier research entailed comparative analyses of human and nonhuman operant behavior; what emerged from this was strong evidence that (a) the performance of verbally able humans on schedules of reinforcement, including concurrent schedules, differed greatly from that observed in nonhuman species, and that (b) a key variable in bringing about these differences was human subjects' ability to specify the contingencies verbally and to formulate their own rules for responding (for reviews see Lowe, 1979, 1983; Lowe, Horne, & Higson, 1987; see also Horne & Lowe, 1993). Understanding how verbal behavior has its effects thus seemed central to analysis of human operant behavior in both basic research and applied settings. When Sidman and colleagues published their landmark studies heralding the phenomenon of *stimulus equivalence* (Sidman et al., 1982; Sidman & Tailby, 1982), we immediately saw this as yet another piece of the puzzle, that is, another phenomenon distinctive to humans, and almost certainly brought about by verbal behavior. One of us (Lowe, 1986)[1] presented a paper to this effect, outlining the evidence for a naming account of stimulus equivalence (see also Lowe et al., 1987). But what our explanation lacked was a clear specification of naming itself, together with an analysis of how it comes about and has its effects on other behavior. It seemed that if such an account could be produced, then this would provide the basis for an new approach to verbal behavior and for an integrated theory of human behavior that would open up domains of experimental research too long neglected by behavior analysis. S. C. Hayes is thus entirely correct in identifying the provision of this broad theoretical base as the main ambition of our paper.

Given the probable scale of such an enterprise, we did not expect to achieve anything like complete success at the first attempt but, emboldened by Kundera's (1988) observation that "All great works contain something unachieved" (p. 65), we considered (although without pretensions to greatness) that even if our particular account of naming acquisition could not be validated in every detail, then at least we could show the kind of account that needs to be established. In any event, and following the detailed scrutiny of so many able commentators, we are pleasantly surprised that so much of our theorizing remains intact and, indeed, in many respects, is broadened and strengthened by our debate with commentators, even those, or particularly those, who have posed the most difficult questions.

What Is a Name and What Is Verbal Behavior?

One of our main aims was to specify what verbal behavior is and to show how it is both similar to and different from other behavior. We identify *naming* as the basic behavioral unit, describing it as a higher order behavior class, combining both listener and speaker relations. Of the 20 or so commentators who addressed this part of the paper, few dissent from our basic account of what constitutes naming (but see de Rose; Chase; McIlvane & Dube; Whitehurst), and none offers an alter-

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[1] Lowe, F. C. (1986, May). *The role of verbal behavior in the emergence of equivalence classes.* Paper presented at the annual meeting of the Association for Behavior Analysis, Milwaukee.

native. The one possible exception to the latter is de Rose, who argues that much of what we seek in the name relation is already in Skinner's *Verbal Behavior* (1957) and, if it is not, then for reasons he carefully outlines, we should not be looking for it. Because we value Skinner's account so highly and have, as a consequence, drawn upon it extensively, we are sympathetic to this critique. However, *Verbal Behavior,* like any great work (and here the epithet is entirely fitting), inevitably falls short of its aims. To take one of many possible examples, it is simply not the case, contrary to what de Rose contends, that either the tact or intraverbal relations in and of themselves can account for "emergent" behavior of the kind observed in match-to-sample tests. There are more fundamental shortcomings that we and others have outlined (see pp. 185–191), but again this should be viewed positively, and *Verbal Behavior* should be seen as providing a vital platform for future theoretical development.

How the Remainder of Our Reply Is Structured

From the commentaries we had hoped to gain fresh evidence from studies and approaches with which we were unfamiliar that might bear upon our main account of verbal behavior, whether for or against. In fact, very few studies or data came to light that were new to us. We also had hoped to see alternative explanations put forward as to how naming might be established or our account further developed; in this respect we were not disappointed. Several commentators have clearly also grappled with these difficult issues and, in many cases, they propose different solutions. Whether we have accepted these or not, they effectively put our theory to the test. Such commentaries, relating to the general naming account, are discussed in the next part of this reply.

In the first paper, we made a number of what we considered to be telling points showing that the construct of equivalence is both redundant and misleading in the field of verbal behavior. Commentaries that have a bearing upon this issue are discussed in the major section beginning on page 326.

The last section of our paper was concerned with the issue that we see as perhaps the most marginal to our main account, but that others consider as of some import; that is, how do organisms come to be successful on match-to-sample tests that are said to measure equivalence? Here we hoped that commentators would consider (a) whether naming is necessary, sufficient, or both, to account for success on these tests, and (b) whether, as we had argued, the construct of equivalence should be abandoned. Few dissent from the view that naming is sufficient (but see p. 333). There are some lively responses on the remaining issues, particularly from equivalence researchers. These issues are discussed in the major section beginning on page 328.

Next, we evaluate how our account stands in relation to Sidman's theory of equivalence and Hayes' relational frame theory, and particularly how, as commentators suggest, all three of these theories might be tested. In the section that follows (pp. 337–339), we consider philosophical and conceptual issues raised in the course of the debate between the commentators and ourselves.

We apologize to all the commentators for landing them, on a very tight schedule, first with what one of them kindly described as a "lengthy treatment" and now with a lengthy reply, the volume of which was determined by the number and variety of points raised. Even so, space and time constraints dictated that not all points raised could be addressed. In general, we devote more space to those commentaries that critique our account than to those that tend to agree with it, because we feel that the debate of such issues is most revealing about the strengths and weaknesses, not just of our views, but also those of critics. To all who have participated we are, accordingly, very grateful, as indeed we are to those working "behind the *JEAB* scene" for enabling it to happen. We hope that the discussion will be of benefit to readers and will help to promote further exploration of these key theoretical issues in the area of verbal behavior.

THE DEVELOPMENT OF VERBAL BEHAVIOR

In his thoughtful commentary, Michael, who accepts the core features of our account of naming and equivalence (but see p. 317), questions, nevertheless, whether we need a

single concept, naming, to embrace the relations of which it is composed. Could we not instead, he wonders, refer to it in terms of the individual repertoires concerned (i.e., listener behavior, echoic and self-echoic behavior, tacting, and conditioned perceptual effects)? One obvious problem here is that terms of this order do not exactly trip off the tongue and so would not play well in Peoria, nor, indeed, in many psychological or other scientific circles. But our attempt at general appeal holds little attraction for Barnes, who argues, correctly, that in using the term *naming* we are seeking popularity for behavior analysis. There is, however, a more fundamental issue lying behind the reservations of both Michael and Barnes, and that concerns whether or not naming specifies a new higher order class in human behavior. We argue below that it does and hence that it merits a distinct term. Whether this need be *naming* is debatable, but whatever the term finally adopted it remains a name by any other name.

What Are Higher Order Classes of Behavior?

Central to our account is the concept of the name as a higher order behavioral relation. However, some of the commentators seem unfamiliar with, or seem not to fully understand, what constitutes a higher order class; others question our specifying the name relation in these terms (Chase; de Rose; Dugdale; McIlvane & Dube; Pilgrim; Michael). Hierarchical relations between behavioral classes, which are a particular feature of verbal behavior (see Catania, in press; Skinner, 1957, p. 313), are undoubtedly complex, and these commentators' questions indicate that we devoted insufficient space to detailing the characteristics of a higher order class of behavior. Fortunately, Catania (in press) furnishes us with a lucid description of the basic features of such a class, as follows:

> a class that includes within it other classes that can themselves function as operant classes (as when generalized imitation includes all component imitations that could be separately reinforced as subclasses). A higher order class is sometimes called a generalized class, in the sense that contingencies arranged for some subclasses within it generalize to all the others. . . . Higher order classes may be a source of novel behavior (e.g., as in generalized imitation of behavior the imitator had not seen before). They also have the property that contingencies may operate differently on the higher order class than on its component subclasses. For example, if all instances of imitation are reinforced except those within one subclass (e.g., jumping whenever the model jumps), that subclass may not become differentiated from the higher order class and so may change with the higher order class rather than with the contingencies arranged for it (i.e., the imitation of jumping may not extinguish even though it is no longer reinforced). Control by the contingencies arranged for the higher order class rather than by those arranged for the subclasses defines these classes; the subclasses may then be said to be insensitive to the contingencies arranged for them. Higher order classes of behavior are held together by the common consequences of their members. (in press)

All the features of a higher order class of behavior as described here (and see commentary by Catania, and Catania, 1992, p. 377), we believe can be identified in naming.

Both listener behavior and the echoic are higher order classes of behavior. It is true of both, for example, that although initial instances of the class must be reinforced, novel instances may thereafter occur in the absence of *explicit* reinforcement; both also serve to generate novel behavior. For example, when a child is taught listener behavior in response to "give me the shoe," "give me the cup," and so on, with each of these listener relations being reinforced, then when the caregiver says for the first time "give me the hat," the child may show the appropriate behavior. This is novel behavior with respect to the hat, and it need never be explicitly reinforced (see p. 196). Similarly, in the case of echoic behavior, once a number of particular echoic relations have been established through reinforcement, new echoic relations emerge, that is, they occur without being explicitly reinforced (Poulson, Kymissis, Reeve, Andreatos, & Reeve, 1991; and see p. 197).

As we have shown (pp. 199–209, *Naming*), when listener and echoic relations combine in the presence of particular objects or events, this creates the conditions for the emergence of a new response class of speaker behavior that is directly evoked by these objects and events. Thus, objects now give rise to speaking and then to listening, that is, re-

orienting to the objects, which in turn reevokes speaker behavior and so on. This closes the circle and establishes a functional unity of these three generalized classes of behavior. At this point the higher order name relation is established. Again, the first instances of this new unit are explicitly reinforced by caregivers. What is now reinforced, of course, is the behavior class as a whole. With each reinforced repetition of the name relation, perhaps as new object class members are encountered (e.g., a new dog, a new chair), naming as a functional higher order class is further strengthened. Thereafter, explicit reinforcement by caregivers for new name relations becomes less important as the automatic reinforcing consequences of naming things become the more potent source of control. For example, when the child, who has already learned the name "chair" including the conventional listener behavior to which it relates (e.g., sitting) names a new object "chair," she can immediately do as others do with that object (e.g., sit on it; see pp. 204–205, 213, *Verbal understanding*). Just how reinforcing naming becomes is indicated by the phenomenon, commonly observed, that young children often do not wait for caregivers to name new objects but instead cue their caregiver's behavior (with contextual cues similar to those previously employed by the caregiver) by pointing to the object and saying, for example, "dat?" or "da?" until the caregiver names it (e.g., Anisfeld, 1984, p. 98). The reinforcing consequences of naming are captured in Palmer's notion of *parity*, which entails the speaker, who is already an accomplished listener, detecting when she conforms or deviates from conventional verbal practices. As Palmer rightly points out, this applies not just to her vocal productions (e.g., as in her changing "tarry" to "carry") but also to her behavior in general.

By the time the child begins to learn some object-speaker relations, listener behavior is already a highly developed generalized class, occurring whenever new objects are named by caregivers even in the absence of explicit reinforcement. Under these conditions, when the child is taught speaker behavior, listener behavior is also established. For example, if while looking at a toy horse the caregiver says to the child "That's a horse. What is it?" this should evoke the child's not only saying "horse" but also listening to */horse/*, that is, orienting to the indicated toy. Indeed, as indicated in our discussion above of higher order listener relations, the generative properties of the child's existing listener repertoire may enable her, if subsequently so instructed, to "point to a horse," "go to a horse," "fetch a horse," and so on.

Is there a problem in accounting for the emergence of listener behavior within the higher order relation? According to Lowenkron (see also Michael), our account has a problem in explaining how listener behavior emerges, and how the name relation is established, when object-speaker relations alone are trained. Our reply is to point out that, whereas the caregiver or, more likely, the researcher, may "intend" to teach only speaker behavior when he or she says "That's a horse. What is it?" nevertheless the conditions that give rise to listener behavior (see above) are also present, making it almost impossible to prevent the child from simultaneously learning listener behavior as well. The establishment of a new higher order name via object-speaker training alone is therefore not a problem for our account; hence, Lowenkron's alternative account, which incorporates the concept of *joint control*, seems to us redundant. Furthermore, there appears to be an additional problem with that account insofar as it requires the child to have learned descriptive autoclitics (i.e., tacting of the variables that evoke her tacting and echoing) before she has yet acquired her first names.

Are operant-based models fundamentally deficient? Some problems that follow from a failure to understand the complexities of operant contingencies, and higher order classes in particular, are evident in Whitehurst's commentary. Whitehurst faults our account as an operant model applied to phenomena that he maintains are substantially driven by associative or Pavlovian processes. He concedes that reinforcement contingencies may be necessary to establish speaker behavior, but operant reinforcement is not, he believes, required to account for the child's learning of a listener repertoire; the latter instead comes about, he argues, by associative processes. The kinds of evidence he adduces to support his view are well exemplified by the Whitehurst, Kedesdy, and White (1982) study described in his commentary, in which children

aged 2 years and older were presented with individual objects accompanied by an adult's instruction incorporating the particular object's name (e.g., "Sandy see the *wick*" or "Sandy the next thing is the *puck*"). This procedure resulted, over a number of sessions, in the child learning to orient (e.g., point) to each named object (i.e., which Whitehurst terms *receptive naming*) and to name the latter (i.e., *expressive naming*), although object-naming lagged behind the related listener repertoires (see Whitehurst et al., 1982). Whitehurst writes, "There were never any contingencies for the child's pointing or labeling" (p. 257). Presumably, "never" refers to the time course of the study and to reinforcers explicitly provided by the experimenters. But had the children's pointing or labeling of other objects *never* been reinforced in their previous 2 years or so of life? That would indeed be an extraordinary claim. As we have shown, it is the child's extensive *early* history of having both her listener behavior and then her naming reinforced that creates the conditions for her subsequent learning of both higher order listener (i.e., receptive) and speaker (i.e., expressive) behavior when she is presented with novel objects that others name, even if her behavior is not then explicitly reinforced. The training conditions in Whitehurst et al.'s study coincide precisely with those that we outline as being sufficient, once higher order naming has been trained, to establish listener and speaker repertoires, regardless of whether there is explicit reinforcement or not. Children speaking in multiword sentences, as were most of the subjects in Whitehurst's studies, have certainly already achieved higher order naming, so there are no difficulties for our account in their having shown untrained receptive and expressive behavior within the limited context of an experiment.

Whitehurst's notion that we consider explicit reinforcers such as "clever girl" to be invariable consequences of behavior is entirely erroneous (see pp. 191–215). It should be noted, however, that reinforcement of some kind, even if not always explicit, may nevertheless be a crucially important determinant of every form of early language development, including listener behavior, echoic responding, and naming (see also comment by Peláez-Nogueras; Bruner, 1974; Kaye, 1982; Poulson et al., 1991). Donahoe and Palmer (1994) have expressed this point very well:

> Only a very small proportion of the contingencies of reinforcement in human affairs is explicit, and verbal contingencies are no exception, as Moerk's analysis reminds us. When a child speaks, adults usually orient to the child and almost invariably respond appropriately in some way. Verbal behavior provides children with tremendous power, which they learn to wield more and more effectively as their repertoire develops. Virtually every instance of verbal behavior is changed with reinforcement of some sort, and very little of it is explicitly arranged. (p. 317)

Pavlovian versus higher order operant accounts of naming. There are important lessons to be learned from the discussion of the Whitehurst commentary. First, if researchers working with children aged 2 years and older are oblivious to the role of implicit and explicit reinforcement histories in establishing and maintaining higher order classes of behavior in those children, they may conclude that operant contingencies are not effective. In addition, insofar as they, the researchers, only manipulate stimulus-stimulus relations (e.g., object-name pairings), they may conclude that Pavlovian processes are dominant in language acquisition. We have tried to show, however, that such conclusions are wholly unwarranted and that verbal behavior, rather than being primarily a collection of Pavlovian conditioned reflexes, is essentially operant behavior, although it incorporates elements of Pavlovian conditioning, as we have indicated in our account.

If it is to succeed in validating either the operant or Pavlovian accounts, experimental research almost certainly needs to be directed at very young infants in the course of learning their earliest listener repertoires through to their acquisition of naming; it needs also to be conducted by researchers attuned to the real-life complexities of reinforcement contingencies. Because the aims of the first half of our paper were to specify the name relation and to show the genesis of such behavior, we have naturally found it most appropriate to focus upon young infants from 6 to 18 months old. We are well acquainted with the studies cited by Whitehurst, but most of these deal with older children; this obvious consideration, rather than theoretical "myo-

pia," is why we have not cited them. (For similar reasons we have not, as Place would have wished us to do, dealt with more advanced forms of verbal behavior such as "sentences.") On a further point of detail, we do not, as Whitehurst contends, maintain that children's pointing is necessary for the establishment of listener behavior; pointing is shown in our figures as only one of many possible examples of listener behavior that the child can learn to produce in response to others' speech.

Most aspects of our account are accepted by Stemmer, and he concurs that early forms of listener behavior at least are operant in nature and are established by differential reinforcement. In what he describes as the *second stage* of listener development, he recognizes, as we do, the emergence of novel combinations of the basic listener repertoire, and he cites similar literature in support. It is with respect to later developments in the listener and naming repertoires (which in his commentary Stemmer unfortunately often conflates, so that it is unclear whether "learning names" refers to the child's learning listener behavior or naming itself) that Stemmer takes us to task. He argues that we ignore *ostensive learning,* which he contends is a special learning process in which it is only necessary to expose the child to object-name pairings for appropriate listener behavior and naming to be established (see our reply to Whitehurst above). Because the first correct response to the vocal stimulus can occur before reinforcement does, he concludes that the process is not an operant one. He suggests that Pavlovian processes are at work but recognizes some difficulties with this account (his attempt to recruit Skinner, 1957, to this cause is based upon a serious misreading; see 1957, pp. 358–362, in which Skinner is clear about the very limited role of conditioned reflexes in such learning, and see de Rose). Stemmer's views on ostensive learning, together with his rationale for adopting this concept and rejecting an operant account, bear many similarities to those of Whitehurst and are thus subject to the same critique we outline above. As with Whitehurst's account, the central problem with Stemmer's position is that it fails to fully appreciate the operation of higher order classes of operant behavior.

What is new about naming? Both Pilgrim and Dugdale raise the question of how naming, particularly if listener and speaker behavior have both been reinforced, might differ from the behavior trained in the McIntire, Cleary, and Thompson (1987) study that claims to find equivalence in monkeys. As papers by Hayes (1989) and Saunders (1989) have described in detail, the procedure of McIntire et al. provided training in which every sample stimulus was discriminative for a particular class-specific pattern of responding, ensuring that the latter was in turn made discriminative for the selection of each of the respective class-member comparisons. The procedure thus directly trained relations that were claimed to have been established in the absence of reinforcement (i.e., emergent relations). The difference between the functional properties of the class-specific pattern of responding in the McIntire et al. study and the name relation can be demonstrated by considering a simple extension of their procedure. This would involve, having once trained conditional relations between the A, B, and C stimuli, all of which are mediated by the class-specific response pattern, training a new conditional discrimination D → C without the mediating response pattern. According to Hayes' (1989) and Saunders' (1989) interpretations of how this procedure works (with which we concur), emergent relations between DA, DB, or CD should not occur. If, on the other hand, following a similar conditional discrimination procedure, a child with established naming skills is taught to common name A, B, and C and then is taught to select C in the presence of D, the naming account predicts that the common name will extend to D, and that DA, DB, and CD will all emerge via the higher order name relation (see Eikeseth & Smith, 1992, and pp. 205–207, 225). This extension of the name is made possible by the child's echoing and reechoing the common name, evoked by the comparison stimulus, C, in the presence of the new sample stimulus, D. McIntire et al.'s monkeys did not have this higher order naming repertoire.

As regards Dugdale's query about the conditions that establish the higher order name relation, we believe that, for any given name relation, it does not matter whether listener and speaker repertoires are brought about by direct or indirect reinforcement, as long as

the unity of speaker-listener relations is established. To those who have asked whether the introduction of naming as a qualitatively new phenomenon is justified (Dugdale; McIlvane & Dube) or whether it might not be better considered in terms of its component repertoires (Michael), we reply that naming is a *qualitatively* new relation because it combines the generalized classes of listener behavior, echoic, and object-speaker behavior within a new higher order unit, and this unit entails, as we have tried to show, unique functional properties that go far beyond those of its independently functioning individual component repertoires. We have given many examples of how entirely different kinds of listener behavior may be directed toward one and the same novel object depending on how it is named (e.g., a child behaves differently with a kitchen bowl she names "boat" than with the same bowl she names "hat"). Indeed, this feature of naming forms the basis for the uniquely human achievement that is termed *rule-governed* or *verbally controlled* behavior (see also pp. 212–213).

Naming and categorization. The commentary by Harnad is intriguing in that we are charged with adopting *associative equivalence* as an approach to explaining naming, whereas, in fact, we have gone to great pains to show that we believe that the concept of equivalence is redundant and that a radically different approach to the study of categorization and naming is required. We are nevertheless chided, along with all others involved in the experimental analysis of behavior, for presenting people with unusual shapes in unusual experimental procedures when we should be finding out how young children come to categorize and name their environment. But given that our paper was written precisely to promote a research enterprise of the latter variety, we must assume that Harnad has misread it. Categorization is indeed complexly determined!

The form of categorization that Harnad identifies as being of central importance to human development is based upon the notion of the abstraction of invariant properties of stimuli, which he exemplifies with a study of humans learning chicken sexing (Biederman & Shiffrar, 1987). This is certainly a fascinating issue, but it is not an especially distinctive feature of human learning as compared to that of nonhuman species. Chickens are good at sexing chickens, and indeed the animal learning literature is replete with examples of various species abstracting invariance from groups of stimuli (and see Catania). But categorization in humans goes much further than this. As Catania indicates, a vast amount of human behavior is concerned with arbitrary classes of disparate stimuli that cannot be categorized on the basis of common features. Even when stimuli do have features in common, other arbitrary factors (e.g., their conventional common function) may serve as the basis for naming and categorizing them. As we have argued throughout the paper, naming is inextricably bound up with the formation of such arbitrary classes. Thus, to understand naming, one must study arbitrary classes; to understand how arbitrary classes come about in verbal humans one must study naming. To confine the research exercise to a study of how organisms learn to discriminate the common physical features of stimuli would be to miss the main target, that being, of course, the infinitely more complex discriminations involved in human language.

Are there species differences in the capacity to form higher order classes? We are asked by Catania, "Could it be that humans differ from other species in their capacity to form higher order classes?" (p. 278). This is possible although, as Catania acknowledges, nonhumans also show evidence of higher order classes. We also ask, could it be that humans differ from other species in their capacity to form the unique higher order class that is naming? We join him in calling for the comparative analyses needed to address these issues.

The Role of Echoic Behavior

According to our account, the echoic is a key component of the name relation (see also Catania; Lowenkron; Michael; K. J. Saunders & Spradlin; Stemmer). As Skinner (1957, p. 56) has observed, it serves to short-circuit what would otherwise be an extremely laborious process of reinforcing progressive approximations to other speaker behavior. It also, however, serves to reinstate the heard stimulus and thereby sustains the listener behavior that is critical in establishing the name relation. When naming is first being learned,

it is echoic rehearsal that enables objects and events to establish control over speaker behavior, and it is this that closes the naming loop. Thus, as each new object or event (e.g., a new chair) is encountered and named by a caregiver, echoic rehearsal (e.g., echoing "chair") in the presence of the named object ensures that the latter thereafter becomes part of that particular name relation. In this manner naming becomes stimulus-classifying behavior. We maintain that the echoic component within naming may be overt or covert; in the case of the latter, as Catania points out, there are some interesting challenges presented for behavior analysis (see also Lowe, 1984; Skinner, 1945). It is also important to note that echoic responding and other speaker behavior need not in any way involve vocalization but can take other forms (e.g., manual or other forms of signing; see pp. 208–209, 240).

The question of why, in our account, echoic behavior does not precede the learning of listener behavior is raised by Catania. We have ordered the phenomena in accordance with the normal sequence in which they occur in childhood. It has been proposed that there are neuroanatomical constraints on the development of operant control of vocal behavior (Baker, 1983; Trevarthen, 1983) that may be responsible in part for the later appearance of the echoic. Interestingly, it has been reported that deaf children learn to produce echoic manual signing as early as 6 months of age, which is some months in advance of the emergence of echoic speech in hearing children (Folven & Bonvillian, 1991).

Is echoic behavior necessary for listener behavior? Clearly it is not, and S. C. Hayes is thus mistaken when he argues that, according to our account, a mute child could not learn to orient to a dog when she hears */Where's the dog?/* because she would be unable to echo "dog." Echoic behavior does not, in fact, feature in our account of listener behavior, and we see no problem, therefore, with mute children, or with nonhandicapped children who have not yet learned to speak, learning listener behavior of that type; indeed, we present many examples of just such listener behavior on the part of infants without speech.

Is there evidence for symmetry or naming without echoic responding? The more central question of the role played by the echoic in emergent behavior, including what he terms *symmetry*, which he claims to have shown occurs in the absence of echoic responding, is also raised by S.C. Hayes. The evidence he cites comes from the study by Lipkens, Hayes, and Hayes (1993) of a 17-month-old child who had already learned some names and had the receptive vocabulary characteristic of a typical 2-year-old. The child was presented with picture-name pairings in which each picture was accompanied by a spoken instruction of the form "This is TAK. Can you say TAK?" When the child uttered the correct name, this behavior was reinforced. In subsequent name-object comprehension tests, when the child was presented with the names spoken by the experimenter (e.g., in the form "Where is TAK?"), he pointed to the correct pictures. But is this really an instance of symmetry, and what, if anything, is emergent? As we have seen in the case of the Whitehurst et al. (1982) study (see also our reply to Stemmer), the object-name pairing procedure employed in the Lipkens et al. study is ideal for establishing name-object "comprehension." Children who, like the subject of the latter study, have already attained listener behavior for hundreds of words, almost certainly also have a long history of not only having looked at pictures (e.g., in picture books) that are named for them, but also of the reinforcement by caregivers of their looking or pointing again at those same pictures when asked, for example, "Where's X?" Again we maintain that, like Whitehurst, Lipkens et al. may have intended to train only object-name relations but, by implicitly capitalizing upon already well-established higher order relations, they directly trained name-object as well as object-name relations. This is not, therefore, a case of symmetry.

Although the Lipkens et al. (1993) study set out, in its first phase, to train speaker behavior (i.e., see object–say name) and then to test for listener behavior (i.e., hear name–point to object), this procedure was not fully successful in establishing speaker behavior. In the second stage of the experiment, they first explicitly trained listener relations (i.e., hear name–point to object) and then tested for the emergence of speaker behavior (i.e., see object–say name). However, interestingly, from the perspective of the present account, no speaker behavior emerged until echoic

training for the particular object names concerned was given off task. This is excellent evidence that echoic behavior is necessary for the establishment of the name relation, which includes emergent object-speaker behavior (and see K. J. Saunders & Spradlin). We have already argued in our paper, however, that the behavior that did emerge in this stage of the Lipkens et al. study cannot be described as symmetry, and we will return to this point below.

In the final experiment of their study, Lipkens et al. (1993) reported that listener behavior (hear name–point to object) and speaker behavior (see object–say name) were each learned over time by exclusion, the acquisition of speaker behavior lagging behind that of listener behavior. Unfortunately, however, the critical tests of listener and speaker behavior (i.e., with novel-novel pictures) were not presented over the time course of the study so that the procedure failed to chart the developmental role of stimulus exclusion processes in the learning of listener and speaker behavior. Nor was the relationship between the subject's developing echoic repertoire and the required speaker behavior assessed at any point. Taken together, these methodological problems mean that we still await a systematic developmental study of the interactions among exclusion learning, echoing, listening, and naming. We agree with S. C. Hayes that this is a key area in which further research needs to be done, and in this respect the Lipkens et al. study has blazed a trail for behavior analysis that we hope others will follow (see Wilkinson, Dube, & McIlvane, in press).

Another Pavlovian account: Can Pavlovian conditioned hearing replace echoic responding? A number of commentators are concerned that we do not give much weight to the notion that objects and events might elicit Pavlovian hearing of words (Barnes; Carr & Blackman; Dugdale; Remington). Dugdale goes so far as to suggest that such conditioning could serve to bring about a bidirectional relation that might enable a child to pass our tests for naming. Object-elicited conditioned hearing does not feature prominently in our account for a number of reasons. First, as some of the commentators note, we do refer to conditioned hearing, and we cite the study by Hefferline and Perera (1963) that suggests that sounds may be "heard" even when there is no external sound source. How we come to see things when there is nothing to be seen or hear things when there is nothing to be heard is an interesting area of study that, as Skinner (1953, 1969) has suggested, may be subject to both Pavlovian and operant control. However, we agree with de Rose and Michael (and see Skinner, 1957, pp. 357–362) that, within the domain of verbal behavior at least, Pavlovian processes probably have a very limited role to play. This can be easily demonstrated.

In the context of early language acquisition, we have described how, when the child hears an object name (e.g., */shoe/*) spoken by her caregiver, this is very often followed by the caregiver pointing to the named object. The heard stimulus may thus be highly predictive for seeing the object and may possibly acquire Pavlovian conditioned properties (Rescorla, 1988), eliciting conditioned seeing. When the child learns listener behavior (i.e., hear */shoe/*–orient to the shoe), this predictive relation becomes even stronger (see pp. 194–195, Figure 5). However, when we consider the possibility that objects might give rise to conditioned hearing of their names, the case seems much less convincing. For example, during the day the child may see the shoes on her feet many times, and see other shoes, but on relatively few occasions might this be followed by the name */shoe/*. Seeing shoes, in other words, would be a poor predictor of hearing their names. So what would under the best of conditions probably be a weak conditioning effect, under these conditions is unlikely to exist at all. Even if intensive training were given to the child whereby whenever her shoe were presented it was followed by the name */shoe/*, with the name never being uttered at any other time and the shoe never being presented without it predicting the auditory stimulus, these effects would be short-lived. For on the very next day after training, life, which ordinarily is so full of shoes that are not predictive of hearing */shoe/*, would continue in its usual way. And this is true of most objects a child encounters. Extinction of any conditioning effects would thus seem inevitable, and this, of course, imposes a major limitation on the credibility of Pavlovian accounts of language, relying as they must upon the hypothesis that

unlikely repeated pairings reinstate the conditioned relations (see Hayes & Hayes, 1992). For these reasons, Dugdale's suggestion that a "name relation" based on classical conditioning could enable a child to pass equivalence tests seems implausible, because even if they were fleetingly established, the conditioned reflexes would rapidly dissipate during unreinforced test trials.

Among the commentators who subscribe to the Pavlovian explanation, Barnes has noticed the flaw in that account. In order to rescue it, along with relational frame theory, he appeals to backward conditioning based upon listener behavior (i.e., hear name–see object). But, as we have already acknowledged, Pavlovian conditioned reflexes to such objects as shoes and their vocal accompaniments may be weak from the outset; the supposed backward conditioning effects, weak as they would be and based on already weak effects, would be unlikely to have any impact on behavior. We are not the first to note the shortcomings of backward conditioning accounts of language (see Hayes, 1994).

Language without speaker behavior? Whitehurst perceives a failure in our account to understand, as he puts it, that "speaking is not necessary for the development of language, including understanding of names" (p. 256). We suspect that there may be some confusion about terms here. Although it is true that we do maintain that *speaker behavior* is necessary for language, we propose that such arbitrary but conventional behavior can take many forms (e.g., manual signing, gestures, etc.). That is, we do not maintain that speaking, in the specific sense of vocalizing, is necessary for language. Nor are there any problems for our account in the fact that children can learn to respond appropriately (i.e., learn listener behavior) to names and more complex word combinations uttered (or signed, etc.) by others before they acquire speaker behavior. Indeed, there is a literature, which we cite (p. 196), that shows that animals too may be capable of such "comprehension."

When speech is delayed, as in the case of the expressive-language-delayed children mentioned by Whitehurst, other forms of verbal production may to some extent substitute for vocalization until it develops. Thal, Tobias, and Morrison (1991) have shown that in such speech-delayed 2-year-old children, the existence of good gestural skills discriminated those who caught up on vocal production skills over a 1-year follow-up from those who remained delayed (and see Whitehurst & Fischel, 1994). Clearly, absence of vocalization does not mean absence of all forms of verbal production (i.e., speaker behavior). Indeed, even deaf children who have not yet been taught a formal signing system learn gestural forms during interactions with their hearing and speaking parents that they employ at first singly and later in combination to name objects and events (Goldin-Meadow, 1982). The anarthic child mentioned by Whitehurst, who was the subject of Lenneberg's (1962) case study, also had some gestural skills, unlike the subjects in other studies of anarthia (e.g., Bishop & Robson, 1989; Cubelli & Nichelli, 1992; Cubelli, Nichelli, & Pentore, 1993; Foley, 1993) who were not only incapable of overt vocalization but, because of their cerebral palsy, found manual signing difficult or even impossible. And yet there is clear evidence (Bishop & Robson, 1989) that at least some of these subjects, when required to recall a sequence of pictures, engaged in covert verbal rehearsal. Comparatively little is known about this subject group, and there is great variation in the brain disorders that give rise to anarthia, but Foley (1993) has presented evidence that indicates that the characteristics of the verbal rehearsal depend upon the individual subject's particular verbal training history. The phonological effects reported by Bishop and Robson (1989), for example, were obtained in Foley's study only with anarthic subjects who had long-term experience with voice output augmentative and attentional communication systems (which, e.g., produce recorded spoken sounds corresponding to each item of printed text that an individual selects, by whatever means). Little further information is provided about the behavioral repertoires of these subjects, and Foley acknowledges that a major problem in this research area is that there is a paucity of information about the early learning histories of the subjects and how covert rehearsal and other verbal skills were established. Contrary to Dugdale, we consider Pavlovian explanations of language development in these subjects to be implausible (see above) and see no reason why their verbal productions should not be viewed as operant behavior and

so incorporated within the higher order naming account. For as we conceive it, almost any operant response class that is accessible to the verbal community can come to serve as the speaker repertoire in the higher order name relation. As we have indicated, however, further behavior-analytic research with subjects who have varying degrees of sensory, speech, and other motor impairments could be particularly helpful in advancing our understanding of the necessary and sufficient conditions for language development.

Listener Behavior

On terms. Michael's commentary raises interesting questions as to how listener behavior might best be categorized. First, he suggests that the single term *listener behavior* might imply that it is a relatively uniform process. This would certainly be unfortunate because, like him, we recognize that the term embraces several different functional relations, both operant and Pavlovian. Indeed, it is likely that a comprehensive functional analysis (such as Skinner attempted with speaker behavior) would produce more than the four categories he lists, and this might well be a fruitful line of inquiry for behavior analysts to pursue. However, what all the categories of behavior involved have in common is their correspondence to conventional verbal stimuli produced by others; hence the common appellation, *listener behavior.* What they also have in common, moreover, is that when the name relation is established, features of any or all of them may become incorporated within it, although precisely what will be incorporated in any particular name relation is an empirical matter. But just as many different kinds of verbal repertoires can be termed *verbal behavior,* so too we see little problem in having a single term for listener behavior as long as we concurrently recognize the need for further analyses of the component repertoires.

In adopting the term *listener behavior,* we have tried to adhere closely to Skinner's usage. Indeed, with all our terms, however complex the relations involved, we have attempted to provide tight behavioral specifications. Inevitably, our particular usages do not always coincide with those adopted in some other accounts, and this can, of course, be a source of possible confusion. L. J. Hayes notes that our definitions of terms are somewhat at odds with those that she has adopted in previous papers, although there is a considerable overlap in our respective approaches to verbal behavior. She requests clarification on a number of points: First, we do distinguish between a child *hearing* sounds and *listener behavior,* it is only the latter that necessarily involves a conventional correspondence between a vocal or other conventional stimulus produced by a speaker and behavior evoked in the listener. Second, in answer to the question, "Does listening involve understanding, or is understanding invoked only when the listener becomes a speaker?" (p. 283), we have used the term *verbal understanding* to describe aspects of the behavior of the speaker-listener; we have not used the term *understanding* for listener behavior per se. To do so would have some odd implications. We might, for example, in preverbal infants, pigeons, or bees, reinforce the behavior of their approaching the experimenter whenever he or she presented the printed word COME HERE, and this behavior may indeed be successfully learned, but what will have been "understood"? To speak of the behavior of nonverbal organisms in terms of understanding seems fraught with difficulties and unlikely to be an advance for behavior analysis. Even as applied to human behavior, the vernacular term *understanding* must be delineated carefully (with the resulting unambiguous patterns of usage carefully maintained) if it is to be helpful as a technical term.

L. J. Hayes' remaining question, "Is naming an act of speaking or an act of listening?" (p. 283; see also McIlvane & Dube for similar uncertainty), raises an issue that is central to our account. The answer is that it is neither the one nor the other in isolation; it is a functional unity of both, that is, speaker-listener behavior (see also Catania). Because we are concerned with naming as the primary and basic unit of verbal behavior and with the speaker as his or her own listener, whether reinforcement for manding is present or not does not present definitional problems for the account. Our characterization of naming as speaker-listener behavior also means, contrary to de Rose's misreading of our position, that for us listener behavior is not verbal behavior.

Peláez-Nogueras appears to accept most of

our account, but believes she differs from us with respect to the behavioral achievements of the preverbal infant. Upon closer scrutiny, however, our differences seem to be more in terminology than substance. Much of the experimental literature she cites on this point overlaps considerably with that which we have cited, and we have no difficulty with the notion that preverbal children form categories or stimulus classes and that these are important for later development of naming; indeed, we argue this very point and have illustrated how stimulus classes are formed via listener behavior (see pp. 195–196, Figure 6). She is right to point out the interesting work being conducted by Gopnik and Melzoff (1992) and others on the causal complexities of the relation between preverbal categorization and name learning; this would undoubtedly be a fruitful area for behavior-analytic research. However, because we wish to maintain conceptual clarity and to retain important distinctions between the repertoires of verbal and nonverbal organisms, we are more reluctant than she is to ascribe to the preverbal infant the capacity for *understanding* (see reply to L. J. Hayes, above), *meaning, reference,* and *equivalence.* In the absence of tight behavioral specifications of these terms, along with the supporting experimental evidence for each, it is difficult for us to determine where, if at all, our respective accounts differ. We certainly are not aware of any experimental demonstration in preverbal infants of stimulus equivalence as defined by Sidman.

Listener behavior and object-related conventional behavior. Like Michael, Hackenberg and Vaidya draw attention to the fact that conventional behavior directed toward objects is not just dependent upon control by the speaker's utterances but is also in part determined by the objects themselves, and they speculate as to just how much of human behavior may be directly object controlled in this way. A major function of language is, we nonetheless contend, to determine when, where, and how one shows conventional behavior with respect to particular objects and events and not to a profusion of others; this controlling role of language is further demonstrated by its capacity to evoke conventional listener repertoires related to a particular object even in the absence of that object. In any given context the extent to which human behavior is under verbal control as opposed to being directly object related is of course a matter for experimental analysis. We would also reply to Hackenberg and Vaidya that it is experimental analysis of this nature, and not just interpretation, upon which we are embarked.

The role of contingencies. The precise contingencies involved in children's learning of listener behavior, together with the echoic and naming itself, are the concerns of much of Chase's commentary. We hope that the information already provided, together with observations made by other commentators (e.g., Michael, Palmer), will help to clarify to some extent the role of reinforcement contingencies in establishing and maintaining these repertoires. It would be foolish to suggest, however, that we have mapped out in detail all the contingencies involved in children's learning names; this is a matter for further experimental analysis. It is regrettable that Chase does not, even briefly, outline the alternative interpretation to our own account that he mentions, particularly given that so little of the experimental research he cites is concerned with verbal behavior in early infancy.

DO THEORIES OF STIMULUS EQUIVALENCE ACCOUNT FOR LANGUAGE?

As stimulus-classifying behavior, naming is clearly much concerned with arbitrary stimulus classes as, of course, is the construct of stimulus equivalence. However, whatever merits the latter might have in other domains (but see below), we argue that its use by Sidman and by Hayes and colleagues in specifying the relation between words and objects or events is profoundly mistaken. Indeed, by our account, there is not a single area of verbal behavior to which it can be successfully applied. Given that so much of the interest in equivalence has been based upon its supposed relevance to language, we expected our conclusions, provocative as they were, to elicit strong counterarguments in defense of the model. To our surprise, few commentators have taken issue with our arguments on this point, and several have in fact accepted them outright.

Is the Relation Between Words and Objects One of Equivalence?

Naming and the equivalence paradigm. In summarizing our account, Catania has rightly observed that we draw a crucial distinction between the stimulus status and response status of words and objects, respectively. It follows from this distinction that there must always be a fundamentally asymmetrical relation between a name and that which is named. K. J. Saunders and Spradlin (and see R. R. Saunders & Green), in an apparent attempt to sidestep this problem, have recast the name relation within a stimulus equivalence framework (depicted in their Figure 1) that they argue strips it of the "unobservable elements" of our account. But far from providing a solution, this elaboration only serves to highlight still further the weaknesses of the equivalence theory. Their Figure 1 purports to show that by separately training a listener relation (i.e., selecting a shoe upon hearing */shoe/*) and an echoic relation (i.e., saying "shoe" upon hearing */shoe/*), it is possible to "derive" the tact relation (i.e., saying "shoe" upon seeing a shoe). The figure looks like the conventional stimulus equivalence triangle, but with the key difference that one of the terms is a response (i.e., saying "shoe") rather than a stimulus; this feature renders the model unworkable. First, in order to test the symmetrical counterpart of the tact relation, how would one present a response? That is, how could one get the child to say "shoe" but not hear */shoe/* before selecting the shoe? This has all of the problems Sidman (1994) has discussed when considering the inclusion of responses as events equivalent to stimuli in a conditional discrimination model (see pp. 227–230 for other difficulties with that formulation). How does one evoke a vocalizing response without a prior stimulus and, if tests for stimulus equivalence nevertheless require a vocalizing response of this sort, is not the prior stimulus, rather than the response it evokes, the determining variable? Second, given that the hear-see relation is trained (i.e., hear */shoe/*–see shoe), a symmetrical relation would imply that whenever the child saw a shoe, she would hear */shoe/*. But we have already shown that there are no grounds to suppose that this occurs. Indeed, even if it did occur, it would be difficult to measure; K. J. Saunders and Spradlin surely would not wish to appeal to unobservable events. In the case of the third relation (i.e., hear */shoe/*–say "shoe"), it is simply not possible to test for the emergence of the symmetrical counterpart because the child hears her own vocalizations. In sum, with or without its unobservable elements, this is probably the most unconvincing equivalence triangle that has yet appeared, although it is perhaps a good example of how the equivalence construct fails to relate to verbal behavior.

Yet another Pavlovian account: Does equivalence between words and objects exist only at the nonskeletal level and can the notion of sameness lie in salivation? Another attempt to rescue the notion that words and objects are equivalent comes from Barnes, who suggests that even if we do not treat the word and object as the same at the overt level (i.e., we do not try to eat the printed word CHOCOLATE), we nevertheless perhaps salivate to both and, in so doing, treat them as the same. Carr and Blackman put forward a similar argument and conclude that we may react in the same way to an object's name as we do to the object itself "at all but the skeletal level" (p. 246). We reply as follows: (a) If this were true, it would mean that, on the level of overt operant behavior at least, equivalence would not exist, a limitation that might not be welcome in many quarters. (b) Many objects (e.g., chairs, tables, carpets), for much of the time, probably do not have these eliciting properties (see Skinner, 1957, pp. 357–362). (c) Common salivation to a word and an object is merely functional equivalence, which is insufficient to yield stimulus equivalence, let alone responding based upon sameness. (d) One of the most telling arguments against this and all other attempts to reduce the relation between words and objects to a simple construct like equivalence is the observation, well known to Mead and Vygotsky and philosophers like Wittgenstein and Ryle, that words have a generalizing function that particular objects do not (p. 234; see also Skinner, 1957, pp. 86–89). In this vital respect, central to any notion of language, words are fundamentally different from the objects and events to which they relate. The point is easily illustrated. Consider encountering a brown-feathered bird as it pecks the ground and walks about. You may regard this animal with interest but

not necessarily salivate. Consider, however, the behavioral effects on you, on another occasion, of someone saying to you "chicken," and how these may vary depending upon whether you have just asked "What's for dinner?" (in which case it may give rise to salivation), said "I'm not going to join your gang," asked "What's this a picture of?" or said "You're my little . . ." The name "chicken" can thus have a myriad different effects that the brown-feathered creature, in and of itself, can never have (cf. Harnad). It is thus both philosophically and behaviorally naive to assume a relation of equivalence between names and the objects or events they name.

Can Relational Frame Theory Account for Language?

Additional arguments that might support a symmetry account of naming and, consequently, a relational frame explanation come from Barnes, Carr and Blackman, and Remington. They all appeal to the possibility that objects give rise to Pavlovian conditioned hearing of the spoken word. The implausibility of this account has already been argued, and it should find favor least of all with the leading proponents of relational frame theory who have usually been highly critical of Pavlovian conditioning accounts of language, particularly those that appeal to weak backward conditioning to explain bidirectional effects (Hayes, 1994; Hayes & Hayes, 1992). Regardless of the suppositions of the Pavlovian account, the notion that before we name or even tact an object we must first hear the object's name seems inherently implausible and unworkable within any theory of language, but especially a behavioral one. Although Skinner (1957), like ourselves (see p. 194), recognizes that both operant and Pavlovian conditioned hearing may occur, he has, pace Remington, never suggested that we have to hear objects' names before we can tact them.

Not only is it the case that object-naming, which is the paradigm case for relational frame theory, cannot be described as symmetry (i.e., mutual entailment), but also, as we have pointed out, it does not involve combinatorial mutual entailment (i.e., transitivity). In the absence of any effective counterargument or evidence, it must thus be concluded that naming is not a relational frame and, hence, that relational frames do not have relevance for accounts of language acquisition.

Rule-Governed Behavior and Verbal Control

In addition to arguing that the relation between names and objects is not one of symmetry or equivalence, we have attempted to show that equivalence fails fundamentally to capture the relation between verbal rules and behavior. None of the commentators takes issue with the validity of these arguments. Taken together with the evidence already discussed above, this confirms our view that the construct of equivalence and the associated concepts of relational frame theory do not help us to account for language acquisition in children.

DOES NAMING ACCOUNT FOR STIMULUS EQUIVALENCE?

The key issues here are whether naming is necessary, or sufficient, or both necessary and sufficient to bring about success on tests that meet Sidman's criteria for stimulus equivalence. Some of the commentators misread our position on these issues, and others are unclear as to how our account might be tested.

The Formalistic Fallacy and Equivalence Research

To clarify these points we shall consider first what we term the *formalistic fallacy* of equivalence research, that is, the assumption that because the test outcomes from different subjects look the same, the determinants of their behavior also must be the same. K. J. Saunders and Spradlin suggest that our treatment of this issue is the most important part of our account, and they have clearly understood the far-reaching implications of the notion for all the work so far done in this area. Quite simply, as they put it, self-instruction may be involved in the performance of many human subjects on equivalence tests (see also Remington). For those who have not participated as subjects in match-to-sample studies or heard subjects outline their verbal rules, one need only consider being a subject in an imaginary study in which the experimenter says of a number of pictures and printed words, "The 'chairs' go together, the 'cats' go together, the 'dogs' go together, and the

'chickens' go together." Might this instruction not affect how the stimuli are named, and might it not form the basis for rules to govern responding? And if rules can be derived from instructions, will they not also be derived by the subjects from elements of the procedure itself and from the subjects' own past histories of naming and otherwise categorizing stimuli (Lowe, 1979)? There is now a considerable weight of evidence and indeed common sense (see Remington) that verbally able subjects performing on match-to-sample tests often talk to themselves, sometimes overtly but most times covertly, about the task and how they should perform on it, and often name the stimuli in a variety of ways that depend, among other things, upon the particular experimental procedure. The important point to be made at this juncture is not that all performance on match-to-sample tests is *necessarily* self-instructed or verbally controlled but that, incontrovertibly, at least some is. The implication that follows from this is that potentially there are at least two kinds of equivalence, that is, one that is verbally controlled and another that is contingency generated.

The evidence, however, indicates that naming and self-instructional repertoires on match-to-sample tests can be many and various, which, taken together with K. J. Saunders and Spradlin's observation that most research in this field is conducted with verbally sophisticated subjects, would mean that much, if not all, equivalence as we know it is not merely one kind of behavioral repertoire but many different kinds of behavior with many different kinds of controlling relations. Yet, as K. J. Saunders and Spradlin put it, "much of the field is proceeding as if everyone is studying the same thing" (p. 308).

Is naming necessary for success on equivalence tests? The formalistic fallacy has its greatest impact when it comes to consideration of the notion of contingency-generated success on equivalence tests. First, to clarify any misconceptions about our position on this matter, we maintain that there are two possibilities: Either (a) it is *necessary* for organisms to have verbal behavior (i.e., naming) before they can pass equivalence tests, or (b) in addition to the different types of verbal behavior that can give rise to success on such tests, there is a different, as yet unspecified, nonverbal route or routes by which nonverbal organisms can "succeed." Contrary to what is asserted by Dickins and Bentall, Fields, and Stromer, our general account of verbal behavior does not preclude the possible occurrence of contingency-shaped test success at some time in some members of some species. That has to be a theoretical possibility and, given the diversity of nature and the abundance of experimental ingenuity, it must remain so until the end of time. It is just that, reviewing reports of years of experimentation with many different animal species and various groups of human subjects, we find no reliable evidence of its having yet occurred. Instead, tried and tested procedures that reliably produce successful outcomes have been conducted over and again with subjects who can at least name the stimuli and in most cases have much more sophisticated verbal skills than basic naming. Again K. J. Saunders and Spradlin, well versed in working with human subjects, observe, "one wonders how much information that is relevant to fundamental issues of symbolization—the issues that Sidman originally sought to define objectively—can be gained from studying verbally sophisticated subjects" (p. 308).

But suppose some nonverbal animal could be shown reliably to pass tests of Sidman equivalence, what would this mean and what would be the implications for accounts of language in general and ours in particular? The reported success of one of the sea lions in the study of visual-visual match to sample by Schusterman and Kastak (1993) serves as an excellent example. Were this finding to be confirmed and other simpler learning processes ruled out, it might be concluded that animals can learn, in some way that is analogous to learning set performance, a higher order behavior that involves stimulus-stimulus reversals, thus enabling them to pass Sidman's tests. But would this be to say that the sea lion has learned language, or naming, or indeed any other skill that has a major impact on human behavior? We have already argued that the equivalence construct does not apply to verbal relations, so it is not at all clear what importance such a learning phenomenon would have in human psychology. Indeed, it is difficult to see how the skill might be of much use to the sea lion or any other nonhuman animal either, which of course may

have a bearing upon why it is so difficult to find in such species.

Is naming sufficient to bring about success on equivalence tests? What is clear from our account, however, is that naming may be *sufficient* to bring about success on equivalence tests. Apart from the developmental evidence showing how naming enables the child to form arbitrary classes of objects and events, the evidence from match-to-sample studies is also compelling; children who repeatedly fail tests pass them when given appropriate naming instructions, with the immediacy of effect typical of verbally controlled behavior. Although some of the commentators speculate about alternative explanations of how verbal interventions work, none denies that they are very effective. It must therefore be reckoned one of the odd ironies of work in this area of behavior analysis that a behavioral variable (i.e., verbal behavior) that is known to bring about success on match-to-sample tests of equivalence, and that can be directly manipulated with major and immediate effects, has been almost completely ignored by researchers in favor of a hypothetical construct that, we maintain, is almost wholly without empirical foundation.

Putting the Naming Account to the Test

We have argued that a virtue of our account, not shared by other theories of equivalence, is that it can be submitted to experimental test. But commentators are divided as to whether our account is indeed testable. Some doubt whether it could be disconfirmed (Dickins & Bentall; Pilgrim) or, indeed, whether any of the three main theories of equivalence are testable (K. J. Saunders & Spradlin). Others maintain that it is disconfirmable and call for further evidence to put it and other accounts to the test (de Rose; Galizio; Remington; R. R. Saunders & Green; Stromer). One commentator (Fields) argues both that it is not confirmable and that further evidence is required to test it. Part of the confusion may arise from the fact, recognized by most commentators, that our general account allows for two distinct possibilities. The first of these, according to which naming is *necessary* for success on equivalence tests, is straightforward and straightforwardly disconfirmable. The second possibility allows, in addition, for a nonverbal route or routes; whether such exist is an empirical matter. But, because, according to our account naming is essentially stimulus-classifying behavior that, once it has been learned, permeates almost all aspects of human performance, we would expect verbally controlled equivalence to be by far the most commonly observed version of equivalence phenomena in humans. As far as nonhuman animals are concerned, because we are not aware of the precise learning processes that might be involved or of the evolutionary benefits that stimulus equivalence would confer, we predict that success on these tests would occur rarely, if ever. We believe that evidence can be gathered, additional to that already in existence, that can test between these possibilities and between our and competing theories. We first review, however, in the light of the commentaries, the evidence bearing upon specific predictions arising from our account.

Can nonverbal animals pass tests for stimulus equivalence? To show that naming is not required for success on equivalence tests, it is only necessary to demonstrate that one nonverbal organism can reliably pass them. In our paper, we reviewed the literature that has investigated this issue with a range of nonverbal species and concluded that, in spite of the steady accumulation of such studies, there has not been any convincing evidence that nonverbal animals can pass tests of stimulus equivalence. We believe, as clearly do a number of the commentators, that the only case of apparent success that merits serious consideration is that of one of the sea lions in the study by Schusterman and Kastak (1993). We do not, as K. J. Saunders and Spradlin claim, reject the evidence of this study, nor do we imply that it is tainted by the reinforcement of test trials. Instead our point is that, partly because of the many false dawns that have accompanied the search for equivalence in nonverbal animals, we are cautious about overinterpreting results from a single sea lion in a single study. In addition, we have pointed out a range of unusual features of the Schusterman and Kastak procedure that might allow for a simpler explanation of their findings than does recourse to the construct of equivalence.

R. R. Saunders and Green and K. J. Saunders and Spradlin express reservations about our conjectures on this score, and, undoubt-

edly, there are issues that require clarification. First, it should be noted that whether the sealion swayed or not in front of the stimuli has little bearing upon our speculations as to how the experimental effects were obtained; our concerns were primarily directed at whether the procedure employed may have resulted in the sealion responding to each sample and correct comparison pair as a compound stimulus. In standard match-to-sample procedures, (a) each incorrect stimulus occurs as often with a particular sample stimulus as each correct stimulus, (b) a response is required to the sample stimulus, and (c) the locations of the trained sample and comparison stimuli are not swapped around, as happens in the reinforced "symmetry" trials in the Schusterman and Kastak study. It is in departing from these standard practices that their procedure may have served to establish the compound stimuli. Because no sample response was required, and incorrect comparison stimuli were constantly varied, the sea lion was effectively presented with a three-stimulus array, only two of which repeatedly predicted delivery of a reinforcer. By altering the location of these two stimuli during "symmetry" training, the location feature of the compound stimulus is minimized and the sea lion has only to move to the outer element of the compound in the three-stimulus array in order for reinforcement to occur. The training procedures were thus well suited for establishing the sample–correct comparison pairs as compound stimuli, regardless of their position in the array, and responding to them as such, the sea lion would be likely to succeed in symmetry test trials. Given that there is already evidence for associative transitivity between elements of compound stimuli, we might well expect that having trained the AB-BA compound and the BC-CB compound in "baseline" trials, then AC-CA relations should be obtained by associative transitivity. The likelihood of this happening is further increased by CA and AC relations being reinforced in the case of the other nontest stimuli. Whether the findings with this sea lion can be replicated and whether our account holds or not or whether a different analysis emerges are matters for future research. In the meantime, caution should be exercised in interpreting these findings as evidence of stimulus equivalence in nonverbal animals.

Claims that studies have shown equivalence in monkeys (McIntire et al., 1987) or either equivalence or symmetry in chimpanzees (Dickins & Bentall; K. J. Saunders & Spradlin) are not convincing, nor has any commentator put forward an argument that might deal with critiques of the work concerned (e.g., Epstein, 1982; Hayes, 1989; Saunders, 1989). Indeed, a number of commentators who appear committed to equivalence as a construct (Galizio; McIlvane & Dube; K. J. Saunders & Spradlin; R. R. Saunders & Green) have provided careful and balanced assessments of the evidence on this issue, and their consensus is that, with the possible exception of the Schusterman and Kastak (1993) study, there is not at present any firm evidence for success on equivalence tests in nonhuman animals. This is not to say that all, or indeed any, of these commentators would accept that nonhuman animals will never pass these tests. On the basis of their own failures with rats (Dube, McIlvane, Callahan, & Stoddard, 1993) and their assessment of problems in studies from animal cognition laboratories (e.g., Urcuioli, Zentall, & De Marse, 1995; Wasserman, De Volder, & Coppage, 1992), McIlvane and Dube conclude that it may never be possible to obtain success on Sidman's tests in small-brained species; hope now lies, as far as they are concerned, with "large-brained" animals. Alone among the commentators, Fields declares that there are data from several studies that indicate equivalence in various species including rats and pigeons (he cites studies, including Dube et al., 1993, that are categorically rejected by McIlvane & Dube themselves as well as by other commentators), but this is merely an assertion that he does not support with either evidence or argument. The overall situation is summarized by Galizio, who observes that any theory will have to account for the fact that nonhuman organisms generally fail the tests for stimulus equivalence but, at present, firm conclusions in this key area may be premature.

Can nonverbal humans pass tests of stimulus equivalence? If it could be shown that any nonverbal human (e.g., young infant) or other human subject who did not, for some reason, name stimuli or use verbal rules during a

study could pass Sidman's tests, then this alone would show that verbal behavior was not necessary for success. The only commentators who claim that such evidence exists are R. R. Saunders and Green, but they merely list the kinds of match-to-sample studies (i.e., those that use abstract stimuli or visual-visual stimuli, or the subjects of which have some learning and language deficits) that we have intensively critiqued. They say that where subjects had reduced verbal repertoires, many were unlikely to engage in verbal behavior of the type observed in normal subjects. This, of course, is not the point; the issue is whether the subjects had sufficient verbal skills to enable them to pass the tests, and whether they did name the stimuli either overtly or covertly. Their additional argument that verbally sophisticated subjects demonstrate rapid emergence of large numbers of untrained relations that would be unlikely if they had to talk their way through all the complex paths by which the stimuli were related presents a very mechanistic view of sophisticated verbal skills, particularly those of inner speech. Carr and Blackman similarly conjecture that subjects in the Eikeseth and Smith (1992) study might not have extended previously trained names to new stimuli because, given the absence of reinforcement, they could not have been "aware" that their original performance improved after training. We have already discussed the complexities of reinforcement for naming; it should also be noted, moreover, that for human subjects in match-to-sample experiments there are often many sources of implicit reinforcement, particularly when they have classified the stimuli correctly and the stimulus configuration is altered and new trial types are introduced (Pilgrim, Chambers, & Galizio, 1995; Saunders & Saunders, 1995[2]). The evidence against naming being involved in human equivalence studies, even where there are reduced verbal skills, thus seems to us far from compelling, a view that is shared by K. J. Saunders and Spradlin, who maintain that those who believe that verbal behavior is not necessary for the demonstration of equivalence "have accepted soft evidence" (p. 307; see also de Rose; Sidman, 1994). They point out that because all the subjects in all these studies exhibited verbal behavior long before they entered the laboratory, the results do not provide hard evidence against the naming account. Indeed, most commentators clearly accept that there is no convincing evidence against the naming account from this domain (de Rose; Galizio; Pilgrim; Remington; K. J. Saunders & Spradlin).

Pilgrim, however, raises the interesting question of why, in the Lowe and Beasty (1987) study, only half of the 3- to 4-year-old children showed equivalence patterns (prior to the verbal intervention) when their naming skills should already have been well developed. Pilgrim recognizes, whereas Dickins and Bentall and Fields do not, that it is not enough to "have naming" to pass visual-visual match-to-sample tests. In the Lowe and Beasty study, for example, all the children could name the individual stimuli within a given class, but they did not have any verbal repertoire (e.g., common naming) for responding to them collectively as a class. It was only when they were provided with sample-comparison intraverbal names that they were successful on the tests. Naming is thus not a talisman that confers on its possessors automatic success on Sidman's tests of equivalence or, indeed, on any other categorization test. For example, a child required to group together all the "dinosaurs" and all the "birds" may not succeed in the task if she has named some of the dinosaurs "birds" or if she cannot name them at all. Each name relates to a class or classes of objects or events, and in any categorization task, such as match to sample, it is the nature of the particular relations established that will determine success or failure. As we have shown, to learn to apply a name to new stimuli or to combine names in intraverbals (as occurred spontaneously for some of the children in the Lowe and Beasty study) in ways that will ensure success on conditional discrimination tests may, in the case of some tasks, require specific training or instructions, even into adulthood.

Is naming sufficient to bring about success on tests of stimulus equivalence? The possibility that naming is both necessary *and* sufficient for success on equivalence tests is supported by evidence, collected from several studies, that

[2] Saunders, R. R., & Saunders, J. M. (1995, April). *The roles of generalised conditional responding and chance in the emergence of equivalence-indicative performances.* Paper presented at the annual meeting of the Experimental Analysis of Behaviour Group, London.

naming interventions are highly effective in bringing about such success. Commentators are generally agreed on the latter, although Galizio did raise the possibility that in the studies concerned, naming training might be confounded with overall exposure to the problem. This was not true of the Lowe and Beasty (1987) study, which employed an across-subjects multiple baseline design to control for such effects (see Beasty, 1987), nor, given how quickly naming can be learned, does it appear to be true of other studies (e.g., Dugdale & Lowe, 1990; Eikeseth & Smith, 1992). However, the possible role of overall exposure is eminently testable.

McIlvane and Dube (see also Fields and Galizio) accept the evidence that naming facilitates successful outcomes on equivalence tests but are unhappy with our ascribing these effects to the name relation itself. Instead, they propose an alternative "naming-as-facilitator-of-prerequisite-discriminations" account and appeal to analogies with observing behavior and attending selectively. The notion that naming simply helps to induce differential attention to the stimuli or generates functional equivalence (and see Galizio) is not supported by the data. For example, several children in the Lowe and Beasty (1987) study could name each individual sample and comparison stimulus, and they learned the prerequisite baseline relations; this clearly indicates that they had attended to the stimuli, but they nevertheless failed tests of stimulus equivalence until given training in intraverbal naming. As McIlvane and Dube know, there are many ways apart from naming in which to establish attention to stimuli in match-to-sample procedures (e.g., differential response requirements; see Sidman et al., 1982), but such interventions do not appear to have anything like the same effects as naming. If they did, there would be no problem in obtaining success on equivalence tasks in subjects who, although they did not name, had the benefit instead of other attention interventions. Still, the concern about attentional effects indicates yet another aspect of the naming account of equivalence that could be put to further test by way of studies designed to directly compare the effectiveness of naming procedures with other interventions designed to enhance attention, observing, or whatever.

In the course of presenting evidence that might disconfirm or confirm the theory, we dealt with many of the equivalence test phenomena commonly reported in the literature. However, Fields (see also Dickins & Bentall) has raised the question, hitherto not addressed, of how we might account for nodal distance effects. If, for example, we were to train, in the following order, the stimulus relations, A → B → C → D → E → F, the order of training might give rise, in verbally skilled individuals (it should be noted that all those who have so far shown nodal distance effects were verbally skilled), to an intraverbal name sequence of the A B C D E F variety. In subsequent equivalence tests spanning a long nodal distance (e.g., F → A), the subject may run through the full length of the intraverbal sequence to determine the F → A relation. Clearly, this would take longer than a test for B → A, where only the first two names of the intraverbal would need to be emitted. Within the training trials of any given study, the order of training and the form of the particular nodal structure will, of course, both enter into the determination of behavior on subsequent tests, but such variations are easily encompassable by the naming account.

S. C. Hayes would like us to explain the findings of the Steele and Hayes (1991) study of high school students learning the relations of same, opposite, or different within the context of a match-to-sample procedure. It seems obvious to us that subjects in this study will have used verbal behavior (i.e., names and rules) to solve the problems posed. But how would we go about finding out about these behavioral repertoires? (To say that they formed relational frames does not appear to us to be informative.) This raises more general issues, also raised by some of the commentators, concerning match-to-sample research of this kind, which applies to the studies of Fields we have just been considering as well as to others. As K. J. Saunders and Spradlin ask (see also Harnad and Remington), how much information can we gain about the determinants of test performance from studying verbally sophisticated subjects, whose categorizing skills are, as Harnad puts it, "overlearned"? We would argue (along with Remington and, hopefully, Harnad) that to capture these determinants effectively, a

developmental functional analysis is required in which researchers may observe the behavioral pieces of the equivalence (i.e., stimulus-classifying) jigsaw come together.

Concluding remarks on existing evidence for and against the naming account of stimulus equivalence. Our conclusion, shared with a number of commentators and based on both the evidence presented by commentators as well as that adduced in our paper, is that the possibility that naming is necessary for subjects to pass tests of stimulus equivalence remains without any serious challenge. And we have shown how this conclusion can be subjected to further experimental test. The second possibility that we have outlined, that is, that there might also be a form of contingency-generated "success" on equivalence tests that did not require verbal behavior, has very little evidence to support it at present. But should instances ever be observed, what would be their implications for our overall account of verbal behavior, and does acknowledgment of the possibility render our account of equivalence untestable? We return to these questions below, after consideration of alternative theories of stimulus equivalence.

THEORIES OF STIMULUS EQUIVALENCE OR BACK TO BEHAVIOR?

In a spirit of harmony no doubt, K. J. Saunders and Spradlin suggest that there may not be a great difference between our account and the theories of Sidman and Hayes (see also Pilgrim). This might be a comfortable view for us to adopt, but it would not be intellectually honest. We have already shown how the accounts differ fundamentally with respect to their specifications of verbal relations, and we have argued in detail that the construct of equivalence, upon which the Sidman and Hayes theories are based, is ill-equipped to deal with verbal behavior. If, however, equivalence theories cannot account for language, perhaps they can, nonetheless, account for stimulus equivalence (i.e., why subjects pass match-to-sample tests).

Hayes' Relational Frame Theory: What Is the History That Establishes Stimulus Equivalence?

One of our main concerns about Hayes' theory is that we find it vague and abstract to the extent that, when not actually misleading, it appears not to be saying very much. To indicate this weakness, in our paper we ask, "What is the history that establishes relational frames and how does it work?" In our account we have detailed a great deal of the behavior that contributes to the development of naming, and we have set out the conditions under which that behavior is learned. Some commentators have called, quite rightly, for still more detail or have challenged specific aspects of the exposition in ways that, if the issues were to be resolved, demanded either further justification of the account or additional research. But Barnes, writing in defense of relational frame theory, criticizes us for providing too much detail and for specifying events that are spatiotemporally contiguous. We confess to having such events in our account, but we point out (a) that even contextualism must allow for some events that are spatiotemporally contiguous, and (b) when the higher order operant that is the name relation is well established, the briefest of cues (e.g., someone saying "a grasshopper") can establish for the child a new full-blown speaker-listener relation that pulls in a whole variety of events, many of which may be distant in space and time. (Although Barnes disputes the fact, contextual cues such as this play a critical role in our account.) Barnes argues that because relational frame theory is a contextual theory, it does not have to specify spatiotemporally contiguous events. Perhaps, but it does have to specify some events. Nowhere is this more necessary than in the specification of the history that might give rise to relational frames. It will not do simply to say that equivalence, naming, and rule following are all forms of operant behavior or relational framing; this certainly will not impress commentators like Harnad, or many others either within or outside behavior analysis.

Barnes sees no problems in the lack of predictiveness of the theory and is puzzled that we ask that the behavioral principles involved be specified (see also S. C. Hayes). Thus, relational frame theory can accept an account of naming development based on private hearing (Barnes), or, if it is found that our account is correct and that speaker behavior is required, then that also could easily be accommodated by frame theory (Barnes and S.

C. Hayes). As K. J. Saunders and Spradlin observe, "relational frame theory ... seems adaptable to any outcome" (p. 308). It seems to us that not to have an account either of the history that brings about framing or of how it works might be overlooked in the short term, but not to know that these explanatory elements are essential indicates a more serious problem.

Like Barnes, S. C. Hayes seems unconcerned about the need to specify the behavioral principles that govern framing, and he believes that it is enough of a behavioral explanation to "point to the histories that give rise to these operants" (p. 310). This, of course, again raises the question of what these histories might be. In fairness to Hayes, he does recognize the problems here and accepts that this is where relational frame researchers need to put their greatest effort. But can the theory that is based upon the notion that equivalence, naming, and framing itself come about as a result of specific histories that establish arbitrarily applicable relational responding be wholly without any example of such a history? As we have noted, the only such example from real life that has ever featured in writings on the theory, and it features in just about all of them, is that of naming acquisition. Despite our careful argument to show that naming is not a relational frame and is not a symmetrical relation, which we consider to be perhaps the most damaging evidence against the relational theory, Hayes has not attempted to counter the points made but instead reasserts that naming is an example of a frame of coordination.

We have already addressed Barnes' attempts to rescue this part of the account by appealing to Pavlovian processes and backward conditioning, and we have noted that similar Pavlovian accounts have already been dismissed by S. C. Hayes in other writings. Our case that naming is not a symmetrical relation still stands. In addition, our observation (p. 232) that combinatorial mutual entailment is not present in the paradigm case of naming has gone unanswered by either Barnes or S. C. Hayes. So given that neither mutual entailment (i.e., symmetry) nor combinatorial mutual entailment (i.e., transitivity) is present, it must be concluded that naming is not an example of a relational frame. The theory thus parts company with accounts of naming acquisition and as a consequence is left without a "history."

Or is it? There is the study of the singular sea lion (Schusterman & Kastak, 1993), which provides perhaps the archetypal history of reinforced relational responding that relational frame theory may be seeking. Whether the results of this study are confirmed or not, they at least do make for an excellent "thought experiment" that tells us a great deal about the theory. According to S. C. Hayes (1994), "relational frames are the defining characteristic of verbal events" (p. 12), and naming is an example of the frame of coordination. Hayes (1994) also apparently accepts Schusterman and Kastak's results, viewing the sea lion's performance on the match-to-sample task as an example of the frame of coordination. It follows from this that the sea lion's behavior must also qualify as verbal behavior, although of what kind is not clear. Were behavior analysis ever to adopt relational frame theory, it would thus be in the somewhat strange position of having to claim that the sea lion had acquired verbal behavior but that (because the relation between names and objects is asymmetrical) in their naming of objects and events children had not. It is to such odd implausibilities that theoretical abstractions, not grounded in data, lead.

With colleagues, particularly L. J. Hayes, S. C. Hayes has identified important shortcomings in behavior analysis and has striven to provide the theoretical innovations that the area needs. His identification of the analysis of verbal behavior, rule governance, and the like as being the "main prize" we wholly endorse, and through his own experimental work (e.g., Lipkens et al., 1993), he has helped to show the way forward. Although we might differ on the value of relational frame theory, there is much that is common in our respective research enterprises and our aspirations for behavior analysis.

Sidman's Theory of Equivalence As a Basic Process

In our paper, we outlined what we find to be serious shortcomings in Sidman's theory of stimulus equivalence (pp. 227–230), to the extent of concluding that the concept of equivalence is not needed. Thus, we were sur-

prised that, although several of the invited commentators have operated within that general theoretical framework for some time and a few of these challenged various aspects of our account of performances on match-to-sample tests, none has replied to our criticisms of Sidman's theory. Perhaps some were unwilling to deal with our critique because it emphasized Sidman's most recent formulations, whereas they have been working with earlier versions. If so, we would observe that Sidman has quite rightly sought to adapt his account to address problems the older version was facing. McIlvane and Dube call for a debate on the proposal that "equivalence is a basic process, not derivable from other processes" (p. 271). Sidman (1994) has presented his position in detail regarding this issue, and our paper includes a critique of that position while offering an alternative. The debate, then, has already begun; we look forward to continuing, constructive discussion of that issue.

Putting the Different Accounts to the Test

It is suggested by K. J. Saunders and Spradlin that it may not be possible to experimentally refute any of the three main theories of equivalence in the near future. This may be unduly pessimistic. We suggest at least two main ways in which differences between the approaches might be put to the test.

The match-to-sample route. First, we suggest that research could be conducted with young preverbal infants using standard visual-visual match-to-sample procedures. If the infants succeeded on the tests of equivalence, then this would clearly show that verbal behavior was not necessary for success on these tests; it would also provide strong support for a Sidman-type account. If, on the other hand, the infants failed the tests, then a procedure could be introduced that would provide reinforcement for repeated reversals of sample-comparison pairs (as occurred in the Schusterman & Kastak, 1993, study, but without some of the other distinctive features of that procedure). If this procedure gave rise to success on the tasks, then this would show that verbal behavior was not necessary and it would provide strong support for the Hayes type of interpretation (but see Boelens, 1994). If both these procedures failed, then the infants could be taught a common name for stimuli in each class (see Dugdale & Lowe, 1990). As a control for overall exposure to the task (Galizio), some of the stimulus classes might be given a common name and others not. To test the notion that name training might have its effects indirectly via attentional factors (Fields; Galizio; McIlvane & Dube), it would also be possible to compare naming with some other interventions designed to enhance attentional selectivity (McIlvane & Dube). If common naming were no more successful than the attentional or control procedures in bringing about success on the match-to-sample tasks, then this would seriously undermine the naming account of equivalence; and not just that naming is necessary for success, but equally, that it is the main route to success. If, on the other hand, the infants were to pass the match-to-sample tests only when they had learned to name the stimulus class members, then both the Sidman and relational frame theories should give way to the naming account. The foregoing experimental tests are devised, of course, on the assumption that we have correctly understood the alternative positions as well as the empirical predictions to which they give rise. They will not be easy studies to conduct, but for those interested in testing the different accounts of equivalence, they should be telling.

The verbal behavior route. Our second and, we believe, much more fruitful, approach is in keeping with the sentiments expressed by S. C. Hayes (see also Harnad), who has proposed that we should not be unduly concerned with experimental minutiae or with trials of strength, but rather should stay focused on the major issue, which is the analysis of verbal behavior and its interactions with nonverbal behavior. Our suggestion then is that we set aside, at least temporarily, our match-to-sample apparatus (see also Galizio; R. R. Saunders & Green; Peláez-Nogueras; Remington) and instead give direct consideration to the verbal behavior of the young child as it occurs in natural environments (see Catania), that is, to the "natural experiment," repeated many times daily, that transforms nonverbal behavior into the extraordinary complexity that is human language. We need to develop methodologies appropriate for the experimental investigation of those verbal and preverbal phenomena, in-

cluding the emergence of classes of arbitrary stimuli, that are of particular interest. Whether match-to-sample procedures are likely to feature much in this methodological armory is unknown, but whatever the methodologies adopted, they should be determined by the demands of the subject matter, rather than the other way around. Tested in this arena of early language development, it should soon become evident which of the three theories is most useful in the experimental analysis of complex human behavior.

PHILOSOPHICAL AND CONCEPTUAL ISSUES

A number of commentators devote considerable space not so much to pointing out discrepancies between our theory and the data or to revealing inconsistencies within our account as to taking us to task for either supposed breaches of the "tradition" or (although not always explicitly stated) apparent violations of the philosophy of science upon which behavior analysis is based. Thus, we have been criticized for (a) including in our account covert events or events that cannot be directly manipulated (Chase; McIlvane & Dube; K. J. Saunders & Spradlin); (b) espousing a mediational account in which behavior, particularly verbal behavior, enters into the determination of other behavior (Chase; Fields; McIlvane & Dube; Stromer); (c) suggesting that there might be significant differences between the behavioral repertoires of humans and nonhuman animals (McIlvane & Dube); and (d) violating the principle of parsimony (Dickins & Bentall; McIlvane & Dube; K. J. Saunders & Spradlin). These all reflect a concern for what are considered to be the special characteristics of behavior analysis and of behavior-analytic interpretation. We shall argue, however, that most of the objections are based upon ill-conceived notions of philosophy of science, particularly the philosophy of science that is radical behaviorism.

Parsimony

For example, parsimony is mentioned by McIlvane and Dube in relation to our suggestion that if nonhumans were successful on tests of equivalence, this might occur via contingency-generated behavioral repertoires that differed from the verbally controlled repertoires typical of humans. They maintain that such a proposal is not "attractive intellectually" (p. 270), but in the same commentary conjecture that animals with big, as opposed to small, brains might pass equivalence tests. But given that the contingency-shaped and rule-governed distinction is already well established in behavior analysis, whereas radical distinctions between the behavioral capacities of big-brained and small-brained animals are almost entirely unknown, which account is the more parsimonious?

Similarly, parsimony is invoked by Fields and K. J. Saunders and Spradlin in their arguments against verbal behavior being regarded as a key variable that might account for success on equivalence tests. Once again, however, although the concept of verbal behavior has a long and honorable history within behavior analysis, the construct of stimulus equivalence that they employ is comparatively new and proving increasingly difficult to fit within behavioral theory. Indeed, Fields argues, in preference to the verbal account, for more parsimonious neural network models (see also McIlvane & Dube), as if such models at present existed that could account for the complexity of human performance on equivalence tests when there is so much uncertainty as to what the behavior is that should be the subject of modeling in the first place.

In brief, the parsimony argument can be used to justify not making any distinctions between the behavioral repertoires of humans and nonhumans, not introducing verbal behavior as a determining variable, not invoking covert events, and not even distinguishing between rule-governed and contingency-shaped behavior. But, as others have observed (Beck, 1957), simplicity cannot necessarily be equated with validity. Indeed, as the above examples show, there is often a narrow dividing line between parsimony and reductionism. Of course, the irony of the present debate in which verbal behavior appears to be the target of charges of nonparsimony is that language has been around for as long as science itself; verbal behavior will not be readily removed from theories of human behavior.

The Fear of Unobservables

There can be little doubt that the task of dealing with covert behavior and covert stim-

uli is not an easy one; yet it is essential to attempt this if significant progress is to be made in the analysis of verbal behavior (see also Catania, Peláez-Nogueras). But some commentators clearly see any reference to covert events as beyond the pale for behavior analysis. For example, K. J. Saunders and Spradlin (see also Stromer) consider this to be a flaw in our account. McIlvane and Dube also refer to problems inherent in the covert behavior involved in the name relation and see our proposal that naming might be automatically reinforced as the beginning of "the slide down the slippery slope," although, with a Kafkaesque twist, they do not say to where. Presumably they would have been happier (see Chase) with observable reinforcers of the "clever girl" variety, which Whitehurst wrongly thinks characterizes our account. But as is evident from the commentaries of Palmer, Peláez-Nogueras, Stemmer, and Whitehurst, restricting allowable reinforcement relations to so narrow a range would certainly be anathema to researchers most familiar with the complexities of verbal behavior in young children. It is precisely because of that complexity that we, as experimenters, have to make inferences about sources of reinforcement that are not always directly observable.

On the other hand, although some verbal behavior can be automatically reinforcing and can exist at a covert level, such behavior is most certainly not free of environmental determinants, nor is it locked away in a private domain that is inaccessible to experimental analysis. For, as we have tried to show, verbal behavior has its origins in the most public of arenas—the verbal community. It begins as overt observable behavior, and although much of it later becomes covert and altered in form, it remains behavior nevertheless, and as such is subject to control by antecedent stimuli and consequences. What is more, although some reinforcement occurs at the covert level, many of the reinforcers that maintain verbal behavior, even when it is covert, come from the environment and are observable. Speaking to ourselves, whether in the form of simple names or in more elaborate self-instructions and rules, has considerable effects on the rest of our behavior so that we tap into a vast range of potent environmental reinforcement. Although complex, all of this is amenable to experimental analysis, particularly an analysis that tracks the developmental course of verbal behavior from overt to covert forms. Indeed, to neglect such an analysis is to open the way to those who claim that language, or aspects of language, are free of reinforcing consequences or indeed any form of environmental determination.

Chase is concerned that our account refers to self-listening, again, presumably, because the phenomenon cannot be directly observed, but also because it cannot be experimentally manipulated; he argues that behavior analysts should not accept such concepts and should not, indeed, accept that behavior can be a causal variable (see also Stromer). In his view, this is a crucial issue for the future of behavior analysis and for what kinds of evidence will be accepted in behavioral theory. Putting aside the problematic connotations of *causal* and any implication that covert events might have a special autonomous causal status, what is at issue here is whether behavior, including covert behavior, can have a determining or controlling role in relation to other behavior. We maintain that it can, and indeed it must, if we are to make sense not only of language acquisition in children but also of all of the phenomena of human operant behavior studied by behavior analysts (Lowe, 1979, 1983). Self-listening is central to our account of the development of verbal behavior so that at least insofar as that account is valuable then the concept is justified. But, as de Rose rightly points out, self-listening is also central to Skinner's analyses of phenomena like autoclitics, self-editing, composition, and thinking, and to his critically important distinction between contingency-shaped and rule-governed behavior. Indeed, it is difficult to imagine what an account of human language and its effects on other behavior would look like if it did not include self-listening.

A virtue of Chase's commentary, however, is that he, at least, appears to be aware that his position is at odds with the basic philosophy of radical behaviorism as formulated by Skinner (see also Lowe, 1984). Skinner's radical behaviorist views on listener behavior, the controlling role of behavior in relation to other behavior, the role of covert behavior and stimuli, including covert reinforcement,

are all neatly spelled out in the following passage:

> Behavior generally stimulates the behaver. Only because it does so can coordinated behavior, in which one response is in part controlled by another, be executed. Verbal behavior exemplifies the coordination which requires self-stimulation. The speaker may be his own listener—for example, when intraverbal responses generate "free association"—and automatic self-stimulation from verbal behavior is crucial in the analysis of syntactical and other processes involved in composition and thinking. We are concerned here with self-tacts—with verbal behavior controlled by other behavior of the speaker, past, present, or future. The stimuli may or may not be private. (1957, pp. 138–139)

and in his definition of rule-governed behavior: "Any actual formulation of the relation between a response and its consequences (perhaps simply the observation, 'Whenever I respond in this way such and such an event follows') may, of course, function as a prior controlling stimulus" (1969, p. 147). We cite these not to appeal to authority but to demonstrate to critics, who have argued that our account is somehow alien to the behavior analysis tradition, that it is in fact, as others have recognized (e.g., Catania, Michael, Palmer), wholly in keeping with that tradition, although undeniably aiming to develop both theory and analysis within it. As we have been led to observe on previous occasions, it is strangely ironic that reference to covert events within behavior-analytic theory should be considered so suspect in principle, given that Skinner himself established the identity of radical, as opposed to methodological, behaviorism largely on the basis of a recognition of the importance of covert events in human behavior (Skinner, 1945, 1953, 1957, 1969). Similarly, Bijou, who has contributed much to the study of child behavior, has shown how the analysis of covert events is both consistent with behaviorist theory and a practical necessity in dealing with problem-solving behavior in children (Bijou, 1976, pp. 70–74; Bijou & Baer, 1967). Each researcher is free, of course, to choose his or her own research strategy, which may or may not embrace an analysis of the role of covert behavior, but it should be clearly recognized that the radical behaviorist thesis, as articulated by Skinner, certainly does not eschew consideration of such events but, rather, maintains that it is folly for science to ignore them (see also Lowe, 1983, 1984).

To underpin our account of the acquisition of verbal behavior in the young child, we have had recourse to a great deal of literature outside the behavior-analytic tradition, in the main because there has been such a marked dearth of relevant research within it. As a consequence our task has been made the more difficult, because many of the theoretical assumptions and methodologies employed in that literature render interpretation of the findings difficult to relate to a behavioral account. This, of course, raises the question of why there should be such a paucity of behavior-analytic work in this area. Some behavior analysts may have been daunted by the sheer complexity of verbal behavior as a subject matter but, in addition, we believe, many have been constrained by an outdated conceptual apparatus that owes rather more to methodological than to radical behaviorism.

CONCLUSION

According to Harnad's commentary, the most that can be accomplished by an experimental analysis of naming behavior, such as that provided by our account, is a specification of the conditions under which people and animals succeed or fail in naming things and the conditions under which bidirectional associations are formed between objects and names of objects. From his scientific perspective, however, more is required from an explanation: "One must also hypothesize and then analyze the internal structures and processes that generate the capacity to exhibit the behavior" (p. 264). This represents yet another "must," another stricture on what is scientifically acceptable, but this time pointing inwards rather than out to the environment. Harnad does not specify the nature of these inner processes. If behavior is allowed, then our account of the covert events involved in naming and the development of inner speech may bring him some comfort. If, however, what he believes is required is a physiological story, then we too are eager to learn more about the physiological underpinnings of the development of verbal behavior in children and to integrate such informa-

tion, as far as possible, into our account. Or perhaps he has another level of explanation in mind? But consider again what Harnad concedes. We maintain, and we have not been the first to do so, that naming is the behavioral atom out of which is generated the vast body of human language, and in this, is responsible for the most profound changes in the structure and function of human behavior. Thus, to give a full account of the necessary and sufficient conditions under which children acquire naming and other verbal behavior, which should indeed be the aim of behavior analysis, would be an achievement with few parallels in science. We certainly should be very happy if our paper and the commentaries it has evoked help in even a small way to further this undertaking.

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